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CONTENTS

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THE EVOLUTION OF BOTANICAL RESEARCH[1]

A MEETING of the American Association in St. Louis is of special interest to botanists. When this city was little more than a frontier town, Dr. George Englemann became one of its citizens. In spite of his duties as a successful physician, he became one of our greatest botanists. In fact, in the days when taxonomy was practically the whole of botany, and our virgin flora was being explored, the great American trio of botanists was Asa Gray, of Cambridge, John Torrey, of New York, and George Englemann, of St. Louis. Englemann's distinction was that he published no general botanical works, but selected a series of the most difficult problems in taxonomy, and in a masterly way organized for us many perplexing groups. With these groups his name will always be associated. To a botanist, therefore, St. Louis means the home of George Englemann.

There is another association also for the botanist. St Louis is the home of one of our great botanical gardens, identified for those of us who are older with the name of Henry Shaw; but we are becoming accustomed to its later name, the Missouri Botanical Garden. Its plans and activities represent a fitting continuation of the spirit of Englemann and Shaw, adapted to the progress of botanical science.

In consequence of these associations, St. Louis may be said to have a botanical atmosphere, of which botanists are very conscious. We have the feeling, therefore, not of a visit, but of a home-coming.

A presidential address, delivered to a group composed of investigators representing all the sciences, and including also those interested

[1] Address of the president of the American Association for the Advancement of Science, St. Louis, December, 1919.

in science should deal with some interest common to all. In my judgment our common bond is interest in research; in fact, the major purpose of this association is to stimulate research by the personal contact of investigators. In selecting as my subject, therefore, the evolution of botanical research, I am assuming that the situation developed may apply in a general way to all scientific research.

My purpose is not to outline the history of botanical research, but rather to call attention to certain evolutionary tendencies and to project them into the future. We are all familiar with the gradual historical development of different phases of botany, until botanists became segregated into many distinct groups, the only common bond being the use of plants for investigation. This segregation was for a time very complete, so that the interests of one group would not have been affected if none of the other groups had existed. This monastic phase of botany has subsided somewhat, not for all individuals, but for the subject in general. The different groups are coming into contact and even interlocking, so that the science of botany bids fair to be recognized as an increasing synthesis, rather than an increasing disintegration. In connection with these gradual evolutionary changes, I wish to emphasize three tendencies which seem to me to be significant. As in all evolutionary progress, the tendencies may seem numerous, but the three I have selected seem to me to be especially prophetic of a new era of botanical research.

1. One of the growing tendencies of botanical research is to attack problems that are fundamental in connection with some important practise. The outstanding illustration, of course, is the increasing attention given to the problems that underlie agriculture; but there are many other practises also which are bedded in botanical investigation. We all realize that this tendency was stimulated by the war; in fact, this has been the experience of all the sciences, more notable perhaps in the case of physics and chemistry than in the other sciences, but a very obvious general result. This tendency is so strong at present, that I do not believe it will ever subside, but it should be understood. There is no evidence that it is tending to diminish research whose sole purpose is to extend the boundaries of knowledge, which all of us must agree is the great objective of research. It merely means that experience developed in connection with an important practise has suggested fundamental problems, whose solution is just as important in extending the boundaries of knowledge as in illuminating some practise. In fact, among our most fundamental problems are those that have been suggested by experience. The injection of such problems among those not related to general experience is not to the detriment of the latter, but simply extends the range of research.

I have no sympathy with the artificial segregation of science into pure and applied science. All science is one. Pure science is often immensely practical; applied science is often very pure science; and between the two there is no dividing line. They are like the end members of a long and intergrading series; very distinct in their isolated and extreme expression, but completely connected. If distinction must be expressed in terms where no sharp distinction exists, it may be expressed by the terms fundamental and superficial. They are terms of comparison and admit of every intergrade. The series may move in either direction, but its end members must always hold the same relative positions. The first stimulus may be our need, and a superficial science meets it, but in so doing it may put us on the trail that leads to the fundamental things of science. On the other hand, the fundamentals may be gripped first, and only later find some superficial expression. The series is often attacked first in some intermediate region, and probably most of the research in pure science may be so placed; that is, it is relatively fundamental, but it is also relatively superficial. The real progress of science is away from the superficial, toward the fundamental, and the more fundamental are the results, the more extensive may be their superficial expression.

Not only are practical problems not a detriment to botanical science, but inciden-

tally they strengthen its claim on public interest as a science that must be promoted. As an incidental result, I look with confidence to a future of far greater opportunity for research than has been possible heretofore, research which must be increasingly fundamental and varied. Even if this were not true, my creed for science is that while its first great mission is to extend the boundaries of knowledge, that man may live in an ever-widening horizon, its second mission is to apply this knowledge to the service of man, that his life may be fuller of opportunity. From the standpoint of science, the second may be regarded as incidental to the first, but it is a very important incident, and really stimulates research. In short, I regard this so-called practical tendency in research as being entirely in the interest of research in general, in increasing the range of fundamental problems, in contributing a powerful stimulus, and in securing general recognition of the importance of research.

2. A second tendency, which I regard as more important, is an increasing realization of the fact that botanical problems are synthetic. Until recently a problem would be attacked from a single point of view, with a single technique, and conclusions reached that seemed as rigid as laws from which there is no escape. In plant morphology, for example, and I speak from personal experience, we described structures, with no adequate conception of their functions. Plant physiologists, on the other hand, would describe functions, with no adequate knowledge of the structures involved; while ecologists often described responses, with no adequate knowledge of either structure or function. The same condition obtained in the other segregates of botany. We all recall the time when plant pathologists described and named pathogenic organisms and paid no attention to the disease, which of course is the physiological condition of the plant. In short, not only taxonomists, but all of us, were simply cataloguing facts in a kind of card index, unconsciously waiting for their coordination. This coordination has now begun, and is one of the strong tendencies which is certain to continue. The morphologist is beginning to think of the significance of the structure he is describing; the physiologist is beginning to examine the structures involved in the functions he is considering; and the ecologist realizes now that responses to environment which he has been cataloguing are to be interpreted only in terms of structure and function. In other words, around each bit of investigation, with its single point of view and single method of attack, there is developing a perspective of other points of view and other methods of attack.

This does not mean a multiple attack on each problem by each investigator. We must remain morphologists, physiologists, and ecologists, each group with its special technique and special kind of data. But it *does* mean a better estimation of the results, a watchful interest in the possibilities of other methods of attack, a general toning down of positiveness in conclusions. We all realize now that plants are synthetic, and that is quite a notable advance from that distant time when we thought of them only as objects subservient to laws of nomenclature. This increasing synthetic view is resulting in a proper estimate of problems. The data secured by each investigation constitute an invitation to further investigation. We have in mind the whole problem and not scraps of information. In short, the synthetic view has developed about our problems the atmosphere in which they actually exist.

3. A third tendency, which seems to me to be the most significant one, is the growing recognition of the fact that structures are not static, that is, inevitable to their last detail. As a morphologist, I may recall to your memory the old method of recording the facts in reference to the development of such a structure as the embryo of seed plants. Not only every cell division in the ontogeny was recorded, but also the planes of every cell division. The conception back of such records was that the program of ontogeny was fixed to its minutest detail. It is probably true that such a structure is about as uniform in its development as any structure can be; but it has become evident now that many of the details recorded were not significant. In-

stead of cataloguing them as of equal value, we must learn to distinguish those that are relatively fixed from those that are variables.

In the same way, much of the older work in anatomy must be regarded as records of details whose relative values were unknown. Even the structures involved in vascular anatomy are not static, but many a phylogenetic connection has been formulated on the conception of the absolute rigidity of such structures in their minutest detail. This conception has made it possible, of course, to develop as many static opinions as there are variables in structure.

Perhaps the greatest mass of details has been accumulated by the cytologists, in connection with their examination of the machinery of nuclear division and nuclear fusion. In no other field has the conception of the rigidity of the structures involved become more fixed, even to the minutest variation in form and position. Of course we all realize that any field of investigation must be opened up by recording all the facts obtained; but we must realize that this is only the preliminary stage. The time has come when even the recorded facts of cytology are being estimated on the basis of relative values; that is, the inevitable things are being differentiated from the variables.

The same situation is developing in the field of genetics. We all recall the original rigidity of the so-called laws of inheritance. It was natural to begin the cultivation of this field with the conception that the program of heredity is immutable, and that definite structures are inevitable, no matter what the conditions may be. There was probably more justification for this conception in this field, on the basis of the early investigations, than in any other, but experience has begun to enlarge the perspective wonderfully. The rapidly accumulating facts are becoming so various that consistent explanations require a high degree of mental agility. More fundamental, however, is the recognition of the fact that the problem of heredity involves not only germinal constitution, which gives such rigidity as there is, but also the numerous factors of environment. In other words, such problems have become synthetic in the highest degree, making possible results that are anything but static.

In considering these illustrations of the tendency to recognize that facts are not all pigeon-holed and of equal value, it is becoming more and more obvious that our botanical problems are in general the application of physics and chemistry to plants; that *laws*, when we really discover them, are by definition static, but that their operation results in anything but static structures. In other words, structure must respond to law, but the particular law that is gripping the situation may be one of many.

With such evolutionary tendencies in mind, what is the forecast for botanical research? I wish to call attention to three important features that seem certain to characterize it.

1. It will be necessary for the investigator who wishes to have a share in the progress of the science, rather than merely to continue the card catalogue assembling of random data, to have a broader botanical training than has seemed necessary heretofore. Our danger has been that the cultivation of a special technique, which of course is necessary, is apt to limit the horizon to the boundary of that technique. In some cases the result to the investigator has been more serious than limiting his horizon; it has led him to discredit other methods of attack as of little importance. In case this attitude is associated with the training of students, it is continued and multiplied by pedigree culture. The product of certain laboratories is recognized as of this type, and it is out of line with the evident direction of progress.

This demand of the future does not mean that one must specialize less than formerly. It is obvious that with the increasing intricacy of problems, and the inevitable development of technique, we must specialize more than ever. What the new demand means is not to specialize less, but to see to it that every specialty has developed about it a botanical perspective. In other words, instead of an investigator digging himself into a pit, he must do his work on a mountain top. This secures some understanding and appreciation

of other special fields under cultivation, some of which will certainly interlock with his own field. To meet this situation will demand more careful attention to the training of investigators than it has received. Interested and even submerged in our own work, as we must be, still we must realize that the would-be investigator must develop his atmosphere as well as his technique, or he will remain medieval.

To be more concrete, the morphologist in the coming days must appreciate the relation that physiology and ecology hold to his own field. This is far from meaning that he must be trained in physiological and ecological investigation; but he must know its possibilities. The same statement applies in turn to the physiologist and ecologist, and so on through the whole list of specialties.

This first forecast of the future applies to the necessary training of investigators rather than to investigation itself.

2. A second important feature that is sure to be included in the botanical investigation of the future is cooperation in research. During the last few years the desirability of cooperation has been somewhat stressed, and perhaps the claims for it have been urged somewhat unduly. This was natural when we were desiring to secure important practical results as rapidly as possible. It opened up, however, the possibilities of the future. No one questions but that individual research, to contrast it with cooperative research, must continue to break the paths of our progress. Men of ideas and of initiative must continue to express themselves in their own way, or the science would come to resemble field cultivation rather than exploration. It is in this way that all our previous progress has been made. The new feature is that individual research will be increasingly supplemented by cooperative research. There are two situations in which cooperative research will play an important rôle.

The more important situation is the case of a problem whose solution obviously requires two or more kinds of special technique. There are many problems, for example, which a morphologist and a physiologist should attack in cooperation, because neither one of them alone could solve it. Two detached and unrelated papers would not meet the situation. Our literature is burdened with too many such contributions now. The one technique must be a continual check on the other during the progress of the investigation. This is a very simple illustration of what may be called team work. It is simply a practical application of our increasing realization of the fact that problems are often synthetic, and therefore involve a synthetic attack.

Another simple illustration may be suggested. If taxonomists and geneticists should work now and then in cooperation, the result might be either fewer species or more species; but in any event they would be better species. The experience of botanists can suggest many other useful couplings in the interest of better results. In the old days some of you will recall that we had investigations of soil bacteria unchecked by any work in chemistry; and side by side with this were investigations in soil chemistry unchecked by any work with soil bacteria.

Perhaps the most conspicuous illustration of discordant conclusions through lack of cooperation, so extreme that it may be called lack of coordination, may be found in the fascinating and baffling field of phylogeny. To assemble the whole plant kingdom, or at least a part of it, in evolutionary sequence has been the attempt of a considerable number of botanists, and no one of them, as yet, has taken into consideration even all the known facts. There is the paleobotanist who rightly stresses historical succession, with which of course any evolutionary sequence must be consistent, but who can not be sure of his identifications, and still less sure of the essential structures involved. History is desirable, but some real knowledge of the actors who make history is even more desirable.

Then there is the morphologist, who stresses similarity of structures, especially reproductive structures, and leaves out of sight not only accompanying structures but also historical succession.

Latest in the field is the anatomist, especially the vascular anatomist, who compares

the vascular structures in their minutest detail, and loses sight of other important factors in any evolutionary succession.

Apparently no one, as yet, has taken all the results from all fields of investigation, and given us the result of the combination. In other words, in phylogeny, we have had single track minds. This has been necessary for the accumulation of facts, but unfortunate in reaching conclusions.

This is but a picture of botanical investigations in general as formerly conducted; and it seems obvious that cooperative research will become increasingly common as cooperation is found to be of advantage.

The second situation in which cooperative research will play an important rôle is less important than the first, but none the less real.

It must be obvious to most of us that our literature is crowded with the records of incompetent investigations. Not all who develop a technique are able to be independent investigators. They belong to the card catalogue class. They are not even able to select a suitable problem. We are too familiar with the dreary rehearsal of facts that have been told many times, the only new thing, perhaps, being the material used; and even then the result might have been foretold. It is unfortunate to waste technique and energy in this way; and the only way to utilize them is through cooperative research, for which there has been a competent initiative, and in the prosecution of which there has been a suitable assignment of parts. In my judgment this is the only way in which we can conserve the technique we are developing, and make it count for something. I grant that the product of such research is much like the product of a factory, but we may need the product.

In one way or another, cooperative research will supplement individual research. Individuals, as a rule, will be the pioneers; but all can not be pioneers. After exploration there comes cultivation, and much cultivation will be accomplished by cooperation.

3. The most important feature that will be developed in the botanical investigation of the future is experimental control. Having recognized that structures are not static, that programs of development are not fixed, that responses are innumerable, we are no longer satisfied with the statement that all sorts of variations in results occur. We must know just what condition produces a given result. This question as to causes of variable results first took the form of deduction. We tried to reason the thing out.

A conspicuous illustration of this situation may be obtained from the history of ecology. Concerned with the relation of plants to their environment, deductions became almost as numerous as investigators. Even when experimental work was begun, the results were still vague because of environment. Finally, it became evident that all the factors of environment must be subjected to rigid experimental control before definite conclusions could be reached.

What is true of ecology is true also of every phase of botanical research. For example, I happen to be concerned with materials that showed an occasional monocotyledonous embryo with two cotyledons, while most of the embryos were normal. The fact of course was important, for it connected up Monocotyledons and Dicotyledons in a very suggestive way, and also opened up the whole question of cotyledony. Important as the fact was, much more important was the cause of the fact. We could only infer that certain conditions might have resulted in a dicotyledonous embryo in a monocotyledon; but it was a very unsubstantial inference. That problem will never be solved until we learn to control the conditions and produce dicotyledonous embryos from Monocotyledons at will, or the reverse. Comparison and inference must be replaced by experimental control; just as in the history of organic evolution, the method shifted from comparison and inference to experimental control. It will be a slow evolution, and most of our conclusions will continue to be inferences, but these inferences will eventually be the basis of experiment. In fact, most of our conclusions are as yet marking time until a new technique enables us to move forward.

These illustrations from ecology and morph-

ology represent simple situations as compared with the demands of cytology or genetics, but the same need of experimental control is a pressing one in those fields. The behavior of the complex mechanism of the cell is a matter of sight, followed by inference, when we know that invisible factors enter into the performance. How the cell program can ever be brought under experimental control remains to be seen, but we must realize that in the meantime we are seeing actors without understanding their action. In fact, we are not sure that we see the actors; the visible things may be simply a result of their action. The important thing is to keep in mind the necessary limitations of our knowledge, and not mistake inference for demonstration.

Even more baffling is the problem of adequate experimental control in genetics. We define genetics as breeding under rigid control, the inference being that by our methods we know just what is happening. The control is rigid enough in mating individuals, but the numerous events between the mating and the appearance of the progeny are as yet beyond the reach of control. We start a machine and leave it to its own guidance. The results of this performance, spoken of as under control, are so various, that many kinds of hypothetical factors are introduced as tentative explanations. There is no question but that this is the best that can be done at present; but it ought to be realized that as yet no real experimental control of the performance has been devised. The initial control, followed by inferences, has developed a wonderful perspective, but a method of continuous control is yet to come.

Having considered the conspicuous evolutionary tendencies of botanical research and their projection into the future, it remains to consider the possible means of stimulating progress. It will not be accomplished by increasing publication. It is probably our unanimous judgment that there is too much publication at the present time. What we need is not an increasing number of papers, but a larger percentage of significant papers. This goes back to the selection of problems, assuming that training is sufficient. A leader is expected to select his own problems, but we are training an increasing army of investigators, and the percentage of leaders is growing noticeably less. There ought to be some method by which botanists shall agree upon the significant problems at any given time, in the various fields of activity, so that such advice might be available. It is certainly needed.

I realize that our impulse has been to treat a desirable problem as private property, upon which no trespassing is allowed. Of course, common courtesy allows an investigator to work without competition; but the desirable problems are still more numerous than the investigators; and we must use all of our investigative training and energy in doing the most desirable things. There need be no fear of exhausting problems, for every good problem solved is usually the progenitor of a brood of problems. We will never multiply investigators as fast as our investigations multiply problems. In the interest of science, therefore, we should pool our judgment, and indicate to those who need it the hopeful directions of progress.

Not only is there dissipation of time and energy in the random selection of problems, but there is also wastage in investigative ability. Every competent investigator should have the opportunity to investigate. The pressure of duties that too often submerge those trained to investigate is a tremendous brake upon our progress. I am not prepared to suggest a method of meeting this situation, but the scientific fraternity, in some way, should press the point that one who is able to investigate should have both time and opportunity. A university regulation, with which we are all too familiar, which requires approximately the same hours of all of its staff, whether they are investigators or not, should be regarded as medieval.

In conclusion, speaking not merely for botanical research, but for all scientific research, it has now advanced to a stage which promises unusually rapid development. The experience of the recent years has brought science into the foreground as a great national asset. It should be one of the func-

tions of this great association to see to it that full advantage is taken of the opportunity offered by the present evolutionary stage of research and public esteem. We must choose between inertia and some display of aggressive energy.

John M. Coulter
University of Chicago

TIME, SPACE, AND GRAVITATION[1]

After the lamentable breach in the former international relations existing among men of science, it is with joy and gratefulness that I accept this opportunity of communication with English astronomers and physicists. It was in accordance with the high and proud tradition of English science that English scientific men should have given their time and labor, and that English institutions should have provided the material means, to test a theory that had been completed and published in the country of their enemies in the midst of war. Although investigation of the influence of the solar gravitational field on rays of light is a purely objective matter, I am none the less very glad to express my personal thanks to my English colleagues in this branch of science; for without their aid I should not have obtained proof of the most vital deduction from my theory.

There are several kinds of theory in physics. Most of them are constructive. These attempt to build a picture of complex phenomena out of some relatively simple proposition. The kinetic theory of gases, for instance, attempts to refer to molecular movement the mechanical thermal, and diffusional properties of gases. When we say that we understand a group of natural phenomena, we mean that we have found a constructive theory which embraces them.

THEORIES OF PRINCIPLE

But in addition to this most weighty group of theories, there is another group consisting of what I call theories of principle. These employ the analytic, not the synthetic method. Their starting-point and foundation are not hypothetical constituents, but empirically observed general properties of phenomena, principles from which mathematical formulæ are deduced of such a kind that they apply to every case which presents itself. Thermodynamics, for instance, starting from the fact that perpetual motion never occurs in ordinary experience, attempts to deduce from this, by analytic processes, a theory which will apply in every case. The merit of constructive theories is their comprehensiveness, adaptability, and clarity, that of the theories of principle, their logical perfection, and the security of their foundation.

The theory of relativity is a theory of principle. To understand it, the principles on which it rests must be grasped. But before stating these it is necessary to point out that the theory of relativity is like a house with two separate stories, the special relativity theory and the general theory of relativity.

Since the time of the ancient Greeks it has been well known that in describing the motion of a body we must refer to another body. The motion of a railway train is described with reference to the ground, of a planet with reference to the total assemblage of visible fixed stars. In physics the bodies to which motions are spatially referred are termed systems of coordinates. The laws of mechanics of Galileo and Newton can be formulated only by using a system of coordinates.

The state of motion of a system of coordinates can not be chosen arbitrarily if the laws of mechanics are to hold good (it must be free from twisting and from acceleration). The system of coordinates employed in mechanics is called an inertia-system. The state of motion of an inertia-system, so far as mechanics are concerned, is not restricted by nature to one condition. The condition in the following proposition suffices: a system of coordinates moving in the same direction and at the same rate as a system of inertia is itself a system of inertia. The special relativity theory is therefore the application of the following proposition to any natural process: "Every law of nature which holds good with respect to a coordinate system K must also hold good for any other system K' provided

[1] From the London *Times*.

that K and K' are in uniform movement of translation."

The second principle on which the special relativity theory rests is that of the constancy of the velocity of light in a vacuum. Light in a vacuum has a definite and constant velocity, independent of the velocity of its source. Physicists owe their confidence in this proposition to the Maxwell-Lorentz theory of electro-dynamics.

The two principles which I have mentioned have received strong experimental confirmation, but do not seem to be logically compatible. The special relativity theory achieved their logical reconciliation by making a change in kinematics, that is to say, in the doctrine of the physical laws of space and time. It became evident that a statement of the coincidence of two events could have a meaning only in connection with a system of coordinates, that the mass of bodies and the rate of movement of clocks must depend on their state of motion with regard to the coordinates.

THE OLDER PHYSICS

But the older physics, including the laws of motion of Galileo and Newton, clashed with the relativistic kinematics that I have indicated. The latter gave origin to certain generalized mathematical conditions with which the laws of nature would have to conform if the two fundamental principles were compatible. Physics had to be modified. The most notable change was a new law of motion for (very rapidly) moving mass-points, and this soon came to be verified in the case of electrically-laden particles. The most important result of the special relativity system concerned the inert mass of a material system. It became evident that the inertia of such a system must depend on its energy-content, so that we were driven to the conception that inert mass was nothing else than latent energy. The doctrine of the conservation of mass lost its independence and became merged in the doctrine of conservation of energy.

The special relativity theory which was simply a systematic extension of the electro-dynamics of Maxwell and Lorentz, had consequences which reached beyond itself. Must the independence of physical laws with regard to a system of coordinates be limited to systems of coordinates in uniform movement of translation with regard to one another? What has nature to do with the coordinate systems that we propose and with their motions? Although it may be necessary for our descriptions of nature to employ systems of coordinates that we have selected arbitrarily, the choice should not be limited in any way so far as their state of motion is concerned. (General theory of relativity.) The application of this general theory of relativity was found to be in conflict with a well-known experiment, according to which it appeared that the weight and the inertia of a body depended on the same constants (identity of inert and heavy masses). Consider the case of a system of coordinates which is conceived as being in stable rotation relative to a system of inertia in the Newtonian sense. The forces which, relatively to this system, are centrifugal must, in the Newtonian sense, be attributed to inertia. But these centrifugal forces are, like gravitation, proportional to the mass of the bodies. It is not, then, possible to regard the system of coordinates as at rest, and the centrifugal forces of gravitational? The interpretation seemed obvious, but classical mechanics forbade it.

This slight sketch indicates how a generalized theory of relativity must include the laws of gravitation, and actual pursuit of the conception has justified the hope. But the way was harder than was expected, because it contradicted Euclidian geometry. In other words, the laws according to which material bodies are arranged in space do not exactly agree with the laws of space prescribed by the Euclidian geometry of solids. This is what is meant by the phrase "a warp in space." The fundamental concepts "straight," "plane," etc., accordingly lose their exact meaning in physics.

In the generalized theory of relativity, the doctrine of space and time, kinematics, is no longer one of the absolute foundations of general physics. The geometrical states of bodies

and the rates of clocks depend in the first place on their gravitational fields, which again are produced by the material systems concerned.

Thus the new theory of gravitation diverges widely from that of Newton with respect to its basal principle. But in practical application the two agree so closely that it has been difficult to find cases in which the actual differences could be subjected to observation. As yet only the following have been suggested:

1. The distortion of the oval orbits of planets round the sun (confirmed in the case of the planet Mercury).

2. The deviation of light-rays in a gravitational field (confirmed by the English Solar Eclipse expedition).

3. The shifting of spectral lines towards the red end of the spectrum in the case of light coming to us from stars of appreciable mass (not yet confirmed).

The great attraction of the theory is its logical consistency. If any deduction from it should prove untenable, it must be given up. A modification of it seems impossible without destruction of the whole.

No one must think that Newton's great creation can be overthrown in any real sense by this or by any other theory. His clear and wide ideas will for ever retain their significance as the foundation on which our modern conceptions of physics have been built.

Albert Einstein

SCIENTIFIC EVENTS

THE ANNUAL REPORT OF THE DIRECTOR OF THE BUREAU OF STANDARDS

A review of the work of the National Bureau of Standards for the year ending June 30, 1919, is given in the alumni report of the director of the Bureau of Standards at Washington. The report describes the functions of the bureau in connection with standards and standardization, and contains a chart and description of the several classes of standards dealt with. The director also gives a clear idea of the relation of the bureau's work to the general public, to the industries, and to the government, and includes a special statement of the military work of the year. Brief statements are made upon practically all of the special researches and lines of testing completed or under way at the bureau. The list of these topics occupies 12 pages in the table of contents.

The bureau is organized in 64 scientific and technical sections and 20 clerical, construction and operative sections. During the year the bureau has issued 51 publications, not including reprintings, 36 of which were new and 15 revisions of previous publications. In the several laboratories of the Bureau more than 131,000 tests were made during the year. The appropriations for the year, including special funds for war investigations, were approximately $3,000,000. A noteworthy event of the year included the completion of the industrial laboratory in which will be housed the divisions having to do with researches and tests of structural materials. The building also includes a commodious kiln house for use, among other purposes, of the ceramics division in the experimental production of new clay products and for general experimental purposes.

The report comprises 293 pages and may be obtained as long as free copies are available by addressing the Bureau of Standards, Washington, D. C.

NEEDS OF THE COAST AND GEODETIC SURVEY

Declaring that the work of the United States Coast and Geodetic Survey, which provides the navigating charts which are the direct means of protecting from loss the vessels of our navy, Coast Guard, and merchant marine, is seriously hampered by lack of funds, the superintendent of the survey makes an appeal for an adequate appropriation to remedy this situation, in his annual report to the secretary of commerce.

In order to make and put these navigational charts into the hands of all who demand them both the field and office forces must be kept up to the highest standards of efficiency, and this can not be done without sufficient funds to maintain and operate modern surveying vessels and obtain able officers and crews to man them. In addition

to the funds needed for the field work of the bureau, larger funds than are now available are required for carrying on the office work, for it is necessary to have highly trained men to prepare and care for the data used in making up these charts.

Lack of money prevents the bureau from obtaining a sufficient number of such men, and many of those at present in the service are leaving for better salaried positions elsewhere. There have been large numbers of resignations from the commissioned personnel and other scientific arms of the bureau, in fact, from all classes of the service, and it is expected that these conditions will continue until something is done to meet the situation.

The superintendent points out that the condition is so serious that it threatens to jeopardize public welfare, for, he says:

The commissioned officers are the lowest paid men of their training in the federal service. Their salaries, compared to those paid in the army and the navy for similar qualifications, are 30 to 50 per cent. less. Much of their work is more hazardous, requires special training, and takes them into all our country's possessions as the pioneer workers or navigators—surveyors who "blaze the trail" on land and sea. And no army or navy officer has greater qualifications, nor do they sacrifice more than the officer of the Coast and Geodetic Survey, yet the latter works for much the lowest salary, gets no longevity pay, no emoluments, and after he has given his best years to the service of his country he must retire without pay.

Too few persons realize the sacrifices a man of ability is making at the present time by remaining in the Coast and Geodetic Survey. Before this country entered the war conditions had grown to a serious stage, but since the signing of the armistice steady disintegration has gone on, and the situation has reached a point where the quality of the Survey's employees is declining principally under the stress of present economic conditions. Unless proper relief is forthcoming at once, and the present salaries are materially advanced, this important branch of the federal government, which has so much to do with the protecting of human lives, will, in a measure at least, be stripped of its best brains.

THE ROYAL MEDALS OF THE ROYAL SOCIETY

As has been noted in SCIENCE these medals were awarded to Professor John Bretland Farmer and Mr. James Haywood Jeans. In conferring them on November 30 Sir Joseph Thomson, the president of the society, said:

Professor Farmer's work is characterized by the fundamental importance of the problems worked upon; thus his memoirs on the meiotic phase (reduction division) in animals and plants are of as great value to zoologists as to botanists, and his conclusions and interpretations of the complex nuclear changes which precede the differentiation of the sexual cells have stood the test of criticism, and remain the clearest and most logical account of these very important phenomena. His papers, in collaboration with his pupil, Miss Digby, on the cytology of those ferns in which the normal alternation of generations is departed from has thrown new light on problems of the greatest biological interest, and especially on the nature of sexuality. In his cytological work on cancerous growths Professor Farmer has established the close similarity between the cells of malignant growths and those of normal reproductive tissue.

Mr. Jeans has successfully attacked some of the most difficult problems in mathematical physics and astronomy. In the kinetic theory of gases he has improved the theory of viscosity, and, using generalized coordinates, has given the best proof yet devised of the equipartition of energy and of Maxwell's law of the distribution of molecular velocities, assuming the validity of the laws of Newtonian dynamics. In dynamical astronomy he took up the difficult problem of the stability of the pear-shaped form of rotating, incompressible, gravitating fluid at a point where Darwin, Poincaré and Liapounoff had left it, and obtained discordant results. By proceeding to a third order of approximation, for which very great mathematical skill was required, he showed that this form was unstable. He followed this up by the discussion of the similar problem when the fluid is compressible, and concluded that for a density greater than a critical value of about one quarter that of water the behavior is generally similar to that of an incompressible fluid. For lower densities the behavior resembles that of a perfectly compressible fluid, and with increasing rotation matter will take a lenticular shape and later be ejected from the edge.

MR. ROCKEFELLER'S GIFTS

THERE were announced on Christmas day two large gifts by Mr. John D. Rockefeller, $50,000,000 to the Rockefeller Foundation and $50,000,000 to the General Education Board, the money to be available for immediate use.

In transmitting the gift to the General Education Board Mr. Rockefeller forwarded this memorandum:

The attention of the American public has recently been drawn to the urgent and immediate necessity of providing more adequate salaries to members of the teaching profession. It is of the highest importance that those intrusted with the education of youth and the increase of knowledge should not be led to abandon their calling by reason of financial pressure or to cling to it amid discouragements due to financial limitations.

It is of equal importance to our future welfare and progress that able and aspiring young men and women should not for similar reasons be deterred from devoting their lives to teaching.

While this gift is made for the general corporate purposes of the board, I should cordially indorse a decision to use the principal, as well as the income, as promptly and largely as may seem wise for the purpose of cooperating with the higher institutions of learning in raising sums specifically devoted to the increase of teachers' salaries.

In reference to this gift, Dr. Wallace Buttrick, president of the General Education Board, makes the following statement:

The general public is well aware that the salaries of instructors in colleges and universities have not thus far, in general, been sufficiently increased to meet the increased cost of living. The General Education Board has since the close of the war received applications for aid from colleges and universities the sum total of which would practically exhaust the working capital of the board.

An emergency exists. It is urgently necessary to take steps to increase salaries in order that men in the teaching profession may be able and happy to remain there, in order that young men and young women who incline to teaching as a career may not be deterred from entering the teaching profession, and, finally, in order that it may not be necessary to raise tuition fees and thereby cut off from academic opportunity those who can not afford to pay increased tuition.

As Mr. Rockefeller's memorandum shows, he recognizes the urgency of the present situation, and has given this large sum to the General Education Board to be used in cooperation with the institutions for the purpose of promptly increasing the funds available for the payment of salaries. It has been the policy of the board to make contributions to endowments, conditioned upon the raising of additional supplementary sums by the institutions aided.

The gifts of Mr. Rockefeller to the General Education Board since its establishment in 1902 have been as follows:

1902	$1,000,000
1905	10,000,000
1907	11,000,000
1909	10,000,000
Total	$32,000,000

The board distributes the interest on the above funds currently and is empowered to distribute the principal in its discretion. Recently Mr. Rockefeller gave the board the sum of $20,000,000 for the improvement of medical education, the interest to be distributed currently and the principal to be distributed within fifty years.

In transmitting the gift to the Rockefeller Foundation Mr. Rockefeller specifically authorizes the trustees to utilize both principal and income for any of the corporate purposes of the foundation which, as stated in the charter, are "to promote the well-being of mankind throughout the world." "While imposing no restriction upon the discretion of the trustees Mr. Rockefeller in his letter of transmittal expresses special interest "in the work being done throughout the world in combating disease through improvement of medical education, public health administration and scientific research." Mr. Rockefeller also alludes to the recent gift of $20,000,000 to the General Education Board to promote general education in the United States, and then adds:

My attention has been called to the needs of some of the medical schools in Canada, but as the activities of the General Education Board are by its charter limited to the United States I understand that gift may not be used for Canadian schools. The Canadian people are our near neighbors. They are closely bound to us by ties of race, language and international friendship; and they have without stint sacrificed themselves, their youth and their resources to the end that democracy might be saved and extended. For these reasons if your board should see fit to use any part of this new gift in promoting medical education in Canada such action would meet with my cordial approval.

This last gift makes the total received by the foundation from Mr. Rockefeller $182,-000,000, of which both income and principal were made available for appropriations. In 1917–18 $5,000,000 from the principal was appropriated for war work.

SCIENTIFIC NOTES AND NEWS

DR. JACQUES LOEB, of the Rockefeller Institute for Medical Research, Dr. Robert Andrews Millikan, of the University of Chicago, Dr. Arthur Gordon Webster, of Clark University, and Dr. W. W. Campbell, of Lick Observatory, have been elected honorary members of the Royal Institution of Great Britain and Ireland.

DR. OTTO KLOTZ, director of the Dominion Observatory, Ottawa, has been appointed the representative of Canada on the "Committee on Magnetic Surveys, Charts and Secular Variation" of the International Geodetic and Geophysical Union, recently formed at Brussels.

DR. C. O. MAILLOUX, who was elected president of the International Electrotechnical Commission for the next period of two years at the plenary meeting in London on October 24, was the president of the American committee. He is the second American to hold that honor. Previous presidents have been Lord Kelvin, Dr. Elihu Thomson, Professor E. Budde and Maurice Leblanc. He is a past-president of the American Institute of Electrical Engineers, and was the first editor of *The Electrical World* serving in that capacity in 1883.

DR. HERRICK E. WILSON, having resigned his position as assistant to Mr. Frank Springer, of the U. S. National Museum, will continue research work upon fossil crinoids at his home in Oberlin, Ohio.

THE American Institute of Baking, founded by the American Association of the Baking Industry, has begun work in Minneapolis under the direction of Dr. H. F. Barnard assisted by an advisory committee of the National Research Council and in cooperation with the Dunwoody Institute. Dr. Barnard has been connected with the State Board of Health of Indiana for nearly nineteen years and was federal food administrator of that state during the war.

DR. PAUL G. WOOLLEY, who recently resigned from the chair of pathology at the University of Cincinnati, is reported to have accepted the direction of a laboratory for medical diagnosis at Detroit.

PROFESSOR A. E. GRANTHAM, for twelve years head of the department of agronomy in Delaware College and agronomist to the Delaware Agricultural Experiment Station, has resigned, his resignation to become effective on February 1, to become manager of the Agricultural Service Bureau of the Virginia-Carolina Chemical Company, with headquarters at Richmond, Va.

DR. L. W. STEPHENSON, of the Geological Survey, has been granted a six months' leave of absence in the early part of 1920, in order to do stratigraphic work for one of the oil companies in the Tampico oil field.

PROFESSOR J. C. MCLENNAN, F.R.S., has resigned as scientific adviser to the British Board of Admiralty, to return to his duties as professor of physics in the University of Toronto.

DR. WICKLIFFE ROSE, general director of the International Health Board of the Rockefeller Foundation, and Dr. Richard M. Pearce, recently appointed director of a new division of medical education, sailed on December 11 for Europe to secure information about public health administration and methods of medical education in England and on the Continent.

DR. THEODORE C. LYSTER, former colonel of the U. S. Army, is now in Mexico representing the yellow fever commission of the Rockefeller Foundation of which General Gorgas is the head.

DR. O. HOLTEDAHL is organizing a Norwegian exploring expedition to Novaya Zemlya, and expects to sail in June. A botanist, a zoologist and a meteorologist will accompany the expedition. Dr. Holtedahl will devote his time to geological and geophysical problems.

At the dedication of the new pathological laboratory of the Philadelphia General Hospital the principal address was delivered by Dr. William H. Welch, of The Johns Hopkins University, who spoke of the important part played by morbid anatomy in the advancement of medicine. Drs. Arthur Dean Bevan, Chicago, and Louis B. Wilson, Rochester, Minn., also spoke.

Nature records the death on November 25 of Frederick Webb Headley, at the age of sixty-three years. Mr. Headley spent nearly forty years of his life as an assistant master at Haileybury College, where he succeeded in maintaining a body of active boy-naturalists in the college. He was the author of "The Structure and Life of Birds" and "Life and Evolution."

UNIVERSITY AND EDUCATIONAL NEWS

Mr. John Markle has agreed to provide the sum of five thousand dollars a year for five years beginning January 1, 1920, for the continuation of the mining engineering course at Lafayette College, which was suspended during the war.

It is planned to establish a school of engineering under the joint direction of the Carnegie Institute of Technology, Pittsburgh, the U. S. Bureau of Mines and the coal operators of the Pittsburgh District.

Delegates from French and Swiss universities met recently at Geneva and made arrangements for interchange of students and professors with credits for corresponding work.

Dr. Meyer G. Gaba, who was an instructor in mathematics at Cornell from 1915 to 1918, has been appointed associate professor of mathematics at the University of Nebraska.

Dr. James Playfair McMurrich, professor of anatomy in the University of Toronto, has been elected dean of the faculty of arts.

Dr. T. Harvey Johnston has been appointed to the new professorship of biology at the Queensland University. Dr. Johnston was one of the traveling commissioners sent abroad by the Queensland government to investigate the Prickly Pear problem.

At the University of Cambridge Dr. F. H. A. Marshall, fellow of Christ's College, has been appointed reader in agricultural physiology, and Mr. P. Lake, of St. John's College, reader in geography.

DISCUSSION AND CORRESPONDENCE

THREAD MOULDS AND BACTERIA IN THE DEVONIAN

While making a comprehensive survey of the comparative histology of the skeletal parts of ancient vertebrates, in conjunction with the study of paleopathology, my attention was attracted to the enlarged and distorted shapes of many lacunae in the carapace of *Borthriolepis* and *Coccosteus*. Closer examination under the oil immersion revealed the occurrence of thread moulds and bacteria in the almost disrupted lacunar spaces, and since these organisms have never before been noted in the osseous elements of such ancient vertebrates, a brief description will be given of them here. There is a great gap in our knowledge of ancient bacteria especially between the Pre-Cambrian bacteria described by Walcott and the Carboniferous forms described by Renault, so that we know nothing of the occurrence of bacteria especially in bony material during the early and middle Paleozoic.

The occurrence of thread moulds (*Mycelites ossifragus*) in the hard parts of invertebrates and vertebrates, from molluscs to man, has been noted for more than eighty years and the literature is very extensive. The canals made by the penetrating moulds, known as the *canals of Roux or Wedl*, have been noted by Kölliker in the hard parts of invertebrates, fossil and recent, by Triepel in recent human bones, by Shaffer in ancient human teeth, by Sonders in a Neolithic skull, by Roux in the skeletal parts of vertebrates, Carboniferous to recent. They have been recently seen in the bony parts of Devonian vertebrates, doubtless they have a very wide distribution and may be regarded as one of the most ancient types of organisms in existence There is nothing peculiar in

their occurrence in the ancient vertebrates except that their course of growth is modified by the histology of ancient bone. In the absence of definite lamellæ the mycelia often seek out a lacuna, enter it and growing out along the direction of the brief canaliculi, expand both the lacuna and canaliculi until the entire structure is disrupted and the canals meet other canals growing out from adjoining lacunæ. In modern human bone the mycelia very often follow the interlamellar spaces, but ancient bone has seldom any definite spaces of this kind and more often is to be regarded as an osteoid substance. That the appearances described for the enlarged lacunæ are not normal is easily checked by a study of normal lacunæ in the adjacent material. A single microscopic field will show both normal and invaded lacunæ. The canals, from 2–4 *micra* in diameter have an undulating course and offer easy channels of entrance to invading bacteria.

The presence of these thread moulds would seem to indicate that the piece of bone showing them was preserved in a moist sandy or muddy place close to the shore, thus agreeing with our previous conceptions of the preservation of fossil material. It is difficult to see how the moulds would find entrance if the material were embedded under sand or silt in deep water. The ancient Egyptian mummies, buried for thousands of years in the dry sand of Nubian deserts do not show such canals, nor do the Cretaceous vertebrates from Kansas show them. Seitz has figured them, though apparently did not recognize their nature, in the bones of Labyrinthodonts and dinosaurs, and I have seen evidences of them in sections from the vertebra of an American sauroped dinosaur.

The bacteria doubtless have entered the bone along the course of the *Canals of Roux* and may be detected at first by the beady, nodular appearance of the canal. Often the bacteria, in *Bothriolepis*, for instance, have invaded a canaliculus which the *Mycelites* did not find. The small clumps, or nodes, may clearly be regarded as colonies of bacteria and doubtless as a form of the *Micrococcus*, described by Renault in the canaliculi of Permian fish bone. The beady appearance of an invaded *canal of Roux* or canaliculus recalls exactly the picture of the invaded dentinal tubules in cases of human dental caries. We are, of course, in this case, as in the case of other ancient phenomena, arguing from the known to the unknown. Here is an ancient situation which parallels a similar modern situation and the argument is sound because on it for over one hundred years we have built the science of paleontology.

These conditions can not be regarded as disease in any sense, but are rather to be regarded as the agents of decay in ancient times. They are the agents of decay and disruption at the present time and from present evidences the same agents of decay have been at work for many millions of years, at least since Devonian times.

ROY L. MOODIE

DEPARTMENT OF ANATOMY,
UNIVERSITY OF ILLINOIS,
CHICAGO

VIBRATION RATE OF THE TAIL OF A RATTLESNAKE

THROUGH the courtesy of Professor H. R. Dill, curator of the natural history museum, opportunity was offered to make a brief study of the rate of vibration of the tail of a diamond back rattlesnake, *Crotalus Adamanteus*. This specimen came from Texas on September 15, 1918, but had been in captivity for some time previously. Its age is not known, as that can not be accurately determined from the number of rattles, some of which are known to have been broken off, and two of the nine or ten remaining are in poor condition. A new rattle is formed with each moulting, a process which has occurred twice during the nine months that the animal has been in the laboratory; the second moulting occurred six months after the first. The snake is about five feet four inches in length and rather thin, since it refuses food. It accepts water, however, and in the latter part of March two sparrows were forcibly fed to it. It is exceedingly alert and vigorous, and frequently strikes at any object that is near its wire cage. It has learned some discretion, and does not risk the resultant bump against the wire unless

rather strongly provoked. Its fangs are intact.

With the aid of two assistants, Mr. Ledieu, who kept the head out of mischief, and Mr. Bunch, who manipulated the apparatus, it was possible to secure a fairly accurate short time record. A Deprez marker, together with a suitable time indicator, was adjusted to trace upon a smoked drum. With one method of recording a small mesh cap of copper wire was fitted over the rattles and connected with a flexible wire through a battery, the marker, and a curved brass plate. Touching the wire cap to the brass plate completed the circuit. With slight provocation vigorous movement resulted and the writer would hold as far back from the tip of the tail as possible and still be able to direct the tip so that it would strike the plate with each complete vibration. Fearing that the cap might be heavy enough to retard the motion, we tried again using a double strand of very fine copper wire wrapped twice around the rattles bringing this wire in contact with the plate as before. The average time of fifty-three consecutive vibrations, with the first method, was 30σ (1σ.001 sec.) with a mean variation of 10σ. The corresponding result for twenty-five vibrations by the second method, was 28σ, with a mean variation of 3.5σ.

To the writer two surprises are contained in this record, the first being the relatively great variability in rate of movement, the extremes ranging from about 10σ to 50σ. After attention was directed to the variations in speed, they become marked even to the unaided ear, although no distinct rhythm can be detected.

The second unexpected result is that the pitch of the tone produced does not depend upon the speed nor upon the constancy of the tail vibration but upon the natural resonance of the rattles themselves. The pitch of this tone, as determined by two musicians with a very keen sense of pitch, and checked with accurately tuned forks, is between C and C♯; the tone is expressed, therefore, by about 128 to 135 vibrations per second. Very marked changes in rate of tail, from the fastest that could be produced by marked provocation, to the almost quiescent state, did not cause a fluctuation of the pitch beyond this approximate half-tone. The tone itself is exceedingly complex however, and it might conceivably vary with the number and size of the rattles. It was possible to detect, but not to identify, certain overtones.

The popular impression that the rattler uses his rattles as a warning that he is about to strike is regarded by Mr. Dill as quite erroneous. This snake, when striking normally does so first and rattles afterward, if at all. It will, for instance, strike at a bird placed in the cage, rattle, then strike again. It appears that the rattle is rather to terrify than to warn. It is also used as a defensive mechanism. The instinct to vibrate the tail is not peculiar to the rattlesnake, but is common to many other species, as, for instance, to the non-venomous king snake and the blue racer.

Mabel C. Williams
State University of Iowa

A TICKET TO ST. LOUIS

I am a schoolmaster. I am not earning a living for myself and family, though my position is counted a good one. I shall be a schoolmaster till I die: I have chosen teaching as my service, and am too old to change. My three sons will not be schoolmasters.

Before the war I was able to make ends meet. I could then devote all my time and energies to the duties of my position. Then came increase of passenger rates, and a war tax added, and I and my family have since stayed home. I even bought several liberty bonds and my children bought war savings stamps at the beginning.

Then came also increased freight rates and of cost of food, and I and my boys began gardening. Then came also increase of wages and decrease of competence in artisans, and I and my boys began doing our own repair work —carpentry, plastering, roofing, ditch-digging, etc. But, staying always home, and raising beans, and fixing spouts is not what I am paid for doing, nor does it get the best results from the long training I have had. And ever since the close of the war I have been vainly hoping to be allowed to devote my time again to my

teaching and research; for I am first and last a schoolmaster.

The war having ended more than a year ago, I thought I should like to go to the meeting of the American Association for the Advancement of Science at St. Louis, to meet my colleagues from the other universities and to talk over plans for the future. Now at the last the poor old decrepit U. S. Railroad Administration, which, I verily believe, has done more than any other single agency to increase the cost of living, decides that this association is not educational! Therefore, its members are not entitled to the reduced fare previously granted to those attending "meetings of religious, charitable, educational, fraternal, or military character." This, the equivalent of 2 cents per mile, which was full fare before the war, may be granted for truly educational gatherings, such as those of public kindergartners; but it is not for such as we are: we pay 3 cents per mile with a war tax added, or we help the railroads by staying at home.

Such is the judgment of a high official in that administration (Mr. Gerrit Fort, assistant director), who is doubtless provided with a salary adequate to support him and his family while he renders such decisions. Hear him: "The term 'educational' taken in its broad sense could be construed to cover a very large number of conventions. It was necessary, therefore, to restrict its definition, and this was done by confining it to those conventions having to do with elementary education, such as meetings of school-teachers."

This is the last straw!

SCHOOLMASTER

SPECIAL ARTICLES

THE PROTECTIVE INFLUENCE OF BLOOD SERUM ON THE EXPERIMENTAL CELLFIBRIN TISSUE OF LIMULUS[1]

IN the preceding communication we showed that the solutions of different salts, which are constituents of blood serum or seawater, differ in their effect on the cellfibrin tissue and that the amount of regenerative outgrowth of the tissue is different in different solutions. If we cover a wound with 5/8 *m* NaCl healing may take place; a small piece of excised placed on a cover-glass and surrounded by a drop of NaCl solution may show a good outgrowth under the conditions of our experiment in which usually a small amount of blood serum was adherent to the piece. However, all of these solutions are inferior to the blood serum of *Limulus*. It was of interest to determine which constituent or combination of substances in the blood serum was responsible for the superiority of the serum, whether it was caused by the balancing action of salts or by another constituent.

Addition of calcium chloride in various quantities to the sodium chloride solution did not improve the latter and usually made it less favorable for the tissue. The addition of seawater in which the inorganic constituents are present in proportions similar to those found in blood serum, prevented an active outgrowth altogether. Inasmuch as it was possible that the alkalinity of the seawater was injurious to the tissue, we used seawater with a hydrogen ion concentration which corresponded to an approximately neutral solution. This did not improve the effect of seawater. The Van't Hoff solution mixture of salts was likewise much inferior to an isotonic NaCl solution. These results made it improbable that the beneficial effect of blood serum was due to inorganic constituents.

This conclusion was corroborated by the effect of the heating of blood serum. Heating the blood serum to 85° for a short time sufficient to coagulate a certain amount of its proteid destroyed the greater part of the beneficial effect of blood serum. Heating this filtered fraction still further to 100° for a short time, and thus producing an additional coagulation, made the blood serum as unfavorable as seawater; such heated and filtered blood serum had still the blue color of normal oxygenated *Limulus* blood. However, how far a proportionality exists between the intensity of heating and of loss of beneficial properties of the serum needs further investigation.

[1] From the Department of Comparative Pathology, Washington University School of Medicine, St. Louis, Mo.

At present we may conclude that the specifically protective effect of blood serum is due not to the combination or inorganic constituents but to the proteid constituents of the blood. This may perhaps explain the fact that different blood sera may differ in their beneficial effect. We even found that the blood sera of diseased, anemic *Limuli* may become as ineffective or as injurious as seawater. Whether the action of microorganisms enters as a factor in the case of blood sera of anemic *Limuli* remains still to be determined.

LEO LOEB

A PRELIMINARY NOTE ON SOIL ACIDITY

WHATEVER may be the cause and nature of soil acidity, apparently part of this acidity is due to some of the materials which constitute the soil itself. This gives rise to the question as to whether the minerals from which the soils are derived are acid; and if not, what changes occur in these minerals to make them acid and what factors cause these changes. Therefore in some work on soil acidity that has recently been done in this laboratory, the problem was attacked along a line somewhat different from that usually followed. Instead of working with acid soils entirely, neutral and basic soils were also chosen and the one factor which probably, more than any other, has to do with the natural changes produced in the soil forming minerals—namely, water leaching through the soil—was investigated. After working with a few soils, it seemed advisable to experiment with the more abundant minerals which constitute certain types of soils, and with a few of their decomposition products.

Such materials as the following were taken for the experiments: soils, rocks, miscellaneous gravel, pure minerals such as quartz, hornblende, microcline and garnet, and some of the decomposition products of the above mentioned minerals and rocks such as silicic acid, kaolin, silica, etc. Nearly all of the rocks, gravel and pure minerals were found to be either neutral or slightly basic. The materials were leached with water containing carbon dioxide, and analyses were made to determine what changes had occurred, both in the samples and in the percolated water.

The results from this work show that of all the samples that were leached, no matter whether the original material was basic or acid, the resulting material was acid; and that with the exception of the decomposition products such as silicic acid, kaolin, etc., nearly all of the samples became more acid. The fact should be emphasized here that all of the materials, with the exception of the soils themselves, were minerals or rocks which contained no organic matter. Hence the acidity was not due to organic matter.

From the above statements, the conclusion may be drawn that the compounds formed from some of the soil-forming minerals due to leaching, are an important factor in making soils acid.

Having shown then that some of the materials of which soils are composed, on being leached with water containing carbon dioxide, make soils acid, the next logical step in this research was to try to determine how these compounds give rise to this acidity.

This problem was attacked by determining the hydrogen ion concentration of neutral water extracts of the materials in question; and by determining the hydrogen ion concentration of similar extracts after different known quantities of standard calcium hydroxide had been added. A curve plotted from the results of these determinations should show (1) any excess of hydrogen ions in the solution; (2) the presence of any compound that is capable of taking up calcium hydroxide as a result of adsorption, by the formation of addition products, or by true chemical action; and (3) any excess of free hydroxyl ions. To illustrate, let the following figure represent the relation between the hydrogen ion concentration (expressed as P_h) in a solution and the amount of calcium hydroxide that has been added. Then line *ab* shows a decreasing excess of hydrogen ions in the solution; *bc* that the hydroxyl ions are being removed from the field of action as fast as they are added; and *cd*, an increasing excess of hydroxyl ions.

The curves plotted from the results of the determinations made on acid soils and on the decomposition products of the soil-forming minerals are similar to the one described above, while those made on neutral or alkaline soils are similar to lines *bc* and *cd* of that curve. This apparently indicates that there are some dissociated acids or acid salts present in the solutions of acid soils, and of the decomposition products; and that with all of the materials some of the calcium hydroxide is entirely removed from the field of action. These statements are interesting, especially when compared with the conclusions drawn in regard to soil acidity from results obtained by the freezing[1] point method. The conclusions

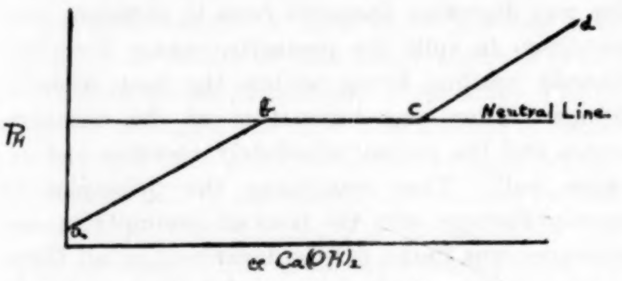

FIG. 1.

by that method are contrary to the former of the above statements, but agree with the latter.

Some other interesting facts concerning these curves are that where they first reach the neutral line, they show a lime requirement as determined by the so-called Jones[2] method; and that where they leave the neutral line, they may indicate what Sharp[3] and Hoagland term "potential acidity" or what Bouyoucos[4] terms "maximum lime requirement." It is also interesting to note that the curves vary somewhat when bases other than calcium hydroxide are added to soils. Barium hydroxide gives rise to curves similar to calcium hydroxide, while sodium and ammonium hydroxides gives curves represented by lines *ab* and *cd* in the above figure.

1 Mich. Agric. Col. Exp. Sta. Technical Bul., No. 27.

2 *Jour. A. O. A. C.*, Vol. I., p. 43.

3 *Jour. Agric. Research*, Vol. VII., p. 123.

4 Mich. Agric. Col. Exp. Sta. Technical Bul. No. 27, p. 37

This work is being continued with the hope that within a short time sufficient data will be obtained to warrant a more complete discussion of the subject.

O. B. WINTER
MICHIGAN AGRICULTURAL COLLEGE,
EXPERIMENT STATION

ALABAMA MEETING OF THE ASSOCIATION OF AMERICAN STATE GEOLOGISTS

ONE of the most successful and profitable annual field meetings of the Association of American State Geologists was held in Alabama, September 1 to 6, 1919, on invitation and under the able guidance of the state geologist, Dr. Eugene A. Smith. Headquarters were at the Tutwiler Hotel, Birmingham.

An instructive printed guide of 14 pages briefly summarizing the essential geologic features to be observed at each place visited in the state was prepared by Dr. Smith and associates. As originally planned, the program called for a division of the party into two sections (Highland and Coastal Plain), to be together only on the first and last days. This plan was later modified to exclude the Coastal Plain section, but was closely adhered to for the Highland section, which closed with a visit to the University of Alabama, so long and well known to geologists as the home of the distinguished host, Dr. Smith.

Much of the Highland region of the state, long known for its varied and complex geology, was covered by excursions, and many of the interesting features of physiography, structure, stratigraphy and economic geology, were reviewed. Among some of the more important localities visited were the famous Birmingham district, where opportunity was afforded for observing some of its more important geologic features, including visits to iron and coal mines, limestone quarries and industrial plants; the extensive productive graphite area between Lineville and Goodwater, the largest domestic producer of graphite; the marble quarries near Sylacauga; and Sheffield and Florence where are located the government nitrate plant and prospective water-power developments at Mussel Shoals on Tennessee River.

The geologists participating in a part or all of the excursions were: Eugene A. Smith and W. F. Prouty (Alabama), J. A. Bownocker (Ohio), G. F. Kay (Iowa), H. B. Kümmel (New Jersey), I. C. White (West Virginia), W. N. Logan (Indiana), S. W. McCallie and J. P. D. Hull (Georgia), W.

O. Hotchkiss (Wisconsin), Collier Cobb (North Carolina), H. F. Cleland (Massachusetts), Herman Gunter (Florida), W. A. Nelson (Tennessee), George Otis Smith, E. O. Ulrich and Charles Butts (Washington, D. C.).

THOMAS L. WATSON,
Secretary

THE AMERICAN CHEMICAL SOCIETY. VII

DIVISION OF BIOLOGICAL CHEMISTRY

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Chemotherapy of organic arsenicals: C. N. MEYERS. A discussion of the transitions of arsenic therapy leading up to the production of salvarsan. A chart showing the methods of approaching the mother substance is presented. The reduction process is briefly discussed, followed by a consideration of the chemical and physical properties, the toxicology, the impurities, and the preservation of salvarsan. The chemical and physical factors as related to the administration of the drug are discussed based upon clinical observations as a result of an extensive investigation of the methods used by leading dermatologists. Standard methods are recommended in order to eliminate reactions which unnecessarily result from faulty technique and improper use of chemical laws when salvarsan is used in organotherapy.

The chemical composition of arsphenamine (salvarsan): G. W. RAIZISS.

A comparative study of the trypanocidal activity of arsphenamine and neo-arsphenamine: J. F. SCHAMBERG, J. A. KOLMER AND G. W. RAIZISS.

Chemotherapeutic studies with ethylhydrocuprein and mercurophen in experimental pneumococcus meningitis of rabbits: J. A. KOLMER AND GORO IDZUMI.

Coordination of the principles of chemo-therapy with the laws of immunity and the successful application in the treatment of tuberculosis: BENJAMIN S. PASCHALL. The tubercle bacillus is protected by waxy substances consisting chiefly of unsaturated highly complex alcohols and equal quantities of phosphatides with which they form a colloidal complex and which in turn exists in close union, possibly physical, more probably chemical, with the protoplasmic substances of the tubercle bacillus, both proteid and carbohydrate in nature. Saponification breaks up this complex without destruction of the important immunizing substances and makes possible separation by solvents. By this means toxic and caseating substances of the Cholin Muscarin group are eliminated as well as the ordinary poisons elaborated by the tubercle bacillus proteins and protein derivatives. Esterification of the fatty acids with ethyl alcohol forms a valuable immunizing substance as these fatty acids have so far been found not to conform to those found in our common food products. Esterification of the higher alcohols with salicylic benzoic, acetic or other suitable acids establishes a new side chain or anchoring group which greatly enhances the reactivity between the antigens themselves and the receptors of the tissue cells so that absorption of these alcoholic esters takes place in the tissues in a few days without producing caseation and tissue necrosis even when given in doses of from 3 to 5 c.c., and following these injections of the mixed esters specific wax digesting ferments form in sufficient concentration to split the protective waxes from the tubercle bacillus living within the host whereby disorganization and destruction of the organism ensues and the patient absolutely recovers and remains well. Thus combining the principles of chemico-therapy with the laws of immunity, a new substance was found for the treatment of all forms of tuberculosis which was successfully used in our own practise and named by us Mycoleum.

The chlorinated antiseptics: Chloramine-T and dichloramine-T: ISAAC F. HARRIS, Ph.D., Research Laboratories, E. R. Squibb & Sons, New York. Toluene-p-sodium-sulfonchloramine (chloramine-T) when prepared in state of high chemical purity is an extremely stable compound, both in crystalline form and in solution. Toluene-p-sulfondichloramine (dichloramine-T) is quite stable when prepared in very high purity chemically dry and protected from dust, organic matter and sunlight. Pure dichloramine-T can be kept in pure, anhydrous chlorcosane, without appreciable decomposition, for several months, if protected from continuous action of direct sunlight. In the reactions between the proteins of the tissues and Dakin's solution, chloramines of the proteins and free sodium hydroxide are formed. The latter furnishes the solvent power attributed to Dakin's solution. When the chloramines react with bacteria and necrotic protein matter, chloramines of the proteins are formed and toluene-p-sulfonamide is set free. The latter is inert and innocuous. The chloramines can be employed with more precision and in greater concentration than Dakin's solution.

An agent for the destruction of vermin-method of application: ALBERT A. EPSTEIN. (By title.)

The purpose of the communication is to put on record the composition of an active vermicide and a suitable method of its application, which was primarily intended for the army. The vermicide is a solution, the base of which is kerosene. The odor and irritating properties of kerosene are disposed of by a special process. To this as a base are added heavy oils and demulcents which promote the retention of the vermicide and repellent properties, by the objects to which the solution is applied. The solution destroys lice within one minute, and nits fail to develop after about eight minutes contact with the solution. As proven by various tests the solution is destructive not only to lice, but to a large variety of insect-parasites affecting man, animals and plants. The solution is applied by means of a spraying device.

An iodine preparation for intravenous and intraspinous use: ALBERT A. EPSTEIN. (By title). It is possible by means of heat under pressure to dissolve native iodine in solutions of dextrine without the aid of the usual solvents. The amount of iodine thus brought into solution bears the approximate relation of 1: 35 to the quantity of dextrine present. The solution thus obtained is homogeneous and fairly permanent. It is strongly bactericidal, its potency ranging from 2½ to 25 times that of the better known antiseptics. Its action is rapid. It is relatively non-toxic when given intravenously and intraspinously. Animals rendered septic by experimental means have been freed of bacteria by intravenous injection of the solution. Clinical application has been made in cases of bacterial endocarditis and typhoid; the clinical course of the disease having been modified by its use. One of the constant effects of intravenous injection is a febrile reaction followed by a very marked leucocytosis. Intraspinous injection has been attempted in tuberculous meningitis. Although the ultimate course of the disease has not been modified by this procedure the solution itself proved to be innocuous. The subject is undergoing further investigation.

The local anesthetic actions of saligenin and other phenolic alcohols: A. D. HIRSCHFELDER, A. LUNDHOLM, H. NORRGARD AND J. HULTKRANS. Since Macht had shown that benzyl alcohol has local anesthetic properties, other members of the phenolic alcohol series, phenylethylalcohol $C_6H_5CH_2CH_2OH$, phenylglycol $C_6H_5CHOHCH_2OH$, cinnamic alcohol $C_6H_5CH=CHCH_2OH$, saligenin $C_6H_4OHCH_2OH$ (salicylic alcohol), methyl saligenin $C_6H_4OCH_3$-CH_2OH, ethyl saligenin $C_6H_4OC_2H_5CH_2OH$, piperonylic alcohol $C_6H_3{<}^{O}_{O}{>}CH_2CH_2OH$, and homosaligenin $C_6H_3OHCH_2OHCH_3$ (1: 2: 4) were investigated. Lengthening of the side chain diminishes the local anesthetic power. Saligenin is the best of the series. It is the least irritating to the tissues, much less so than benzyl alcohol. It is only half as toxic as the latter, longer and in half the concentration. It is a practical surgical anesthetic, and in six tonsillectomies and one tumor removal in man proved to be as good as procaine. Lethal dose for man would be more than a liter of 4 per cent. solution. Covering the phenolic hydroxyl diminishes the local anesthetic power. Homosaligenin is a good local anesthetic, but more irritating.

The effects of drugs which inhibit the parasympathetic nerve endings upon the irritability of intestinal loops: A. D. HIRSCHFELDER, A. LUNDHOLM H. NORRGARD AND J. HULTKRANS. Drugs which inhibit the parasympathetic nerve endings, such as atropin, amyl nitrite, benzyl alcohol, benzyl benzoate and saligenin cause a definite elevation of the threshold of irritability of loops of intestine to intermittent electrical stimuli. The normal rabbit's intestine responds with an annular contraction to a stimulus from a Harvard induction coil at 10 to 12 cm. After painting the mesenteric border of the intestine with any of the above-mentioned drugs in 2 per cent. solution or emulsion the stimulus must be raised to one with the coil at 4 cm. This rise in the threshold, or decrease in the irritability, is probably due to the transition from response by the nerve to response by the muscle after the nerve impulse has been blocked. The same strength of impulse was required after all the paralyzing drugs.

The effect of fever upon the action and toxicity of digitalis: A. D. HIRSCHFELDER, J. BICEK, F. J. KUCERA AND W. HANSON. The action of the drug was studied in cats and frogs whose body temperature had been raised by immersion in a water-bath. Increasing the body temperature in both cats and frogs diminished the size of the dose necessary to cause death. This is less marked at the lower ranges of temperature than in the higher temperatures, and it is most marked within one or two degrees of the thermal death-point of the animal. At 41° the lethal dose for cats is not reduced, at 42° it is one half to two thirds the normal, at 43° it is only one third to one half the lethal dose in normal animals. This proves the necessity of caution in the administration of large doses of digitalis to patients with high fever.

The toxicity of tobacco smoke from cigars, cigarettes and pipe tobacco: A. D. HIRSCHFELDER, A.

E. Lange and A. C. Feaman. Previous investigators had shown that the amount of nicotine in the smoke from a cigar or a cigarette or from smoking pipe tobacco bears no relation to the nicotine in the tobacco itself. "Light" tobacco may give smoke rich in nicotine, "strong" tobacco may give smoke poor in nicotine. Storm van Leuven in Holland showed that smoke from the so-called nicotine-free cigars gives a smoke that contains a good deal of nicotine. Since nicotine is not the only poisonous constituent of smoke, Hirschfelder and his collaborators studied the poisonous action of the smoke itself, or rather the poisonous action of extracts made from passing the smoke through salt solution and through ether. The amount necessary to kill a frog was determined. Using several popular-priced brands of cigar, cigarette and pipe tobacco, it was found that the smoke coming from a given weight of tobacco varied somewhat, but not very greatly in its poisonous action on frogs. When the same weight of the same sample of tobacco was smoked in the form of a cigarette and in a pipe and as a cigar there was sometimes very little difference in the poisonous quality of the smoke, but usually that which was smoked as a cigarette was somewhat less poisonous. Nevertheless, cigars and pipes seem much stronger than cigarettes. This is because since the burning occurs chiefly along the surface of the tobacco, so much more tobacco is being converted into smoke at each instant in these than in the cigarettes. It is largely a question of cross section. Cigars have about four times the cross section of cigarettes, pipes nine or ten times. If all three were smoked equally fast, the smoker would get an overwhelming dose of nicotine from cigar and pipe. Therefore, these must be smoked more slowly than the cigarette and can not be inhaled. If the smoker did not inhale the smoke, the cigarette would be the lightest form of tobacco.

Some applications of protein chemistry to medicine and pharmacy: I. F. Harris.

Action of trichlorotertiary butyl alcohol (chloretone) on animal tissue: T. B. Aldrich and H. C. Ward. The action of chloretone on animal tissue has not been studied, although glands of various kinds have been preserved in a sterile condition in chloretone water for a number of years, without any apparent injury to the active principles they contain. In order to test the action of a saturated aqueous solution of chloretone on animal tissue pieces of various organs were removed from the animal (dog) as quickly as possible after death, cut into small pieces and distributed among several sets of bottles containing water saturated with chloretone. One set was kept at 37°, one at 15°, while others at summer room temperature. One set at room temperature was inoculated with *B. Proteus.* Control tissue with only distilled water showed a high degree of putrefaction in two days. Every few days the tissues were examined and the general appearance, color, odor, etc., noted. In general the tissues became soft and spongy and lost much of their normal color. There was at no time a suggestion of putrefaction. In fact, cultures made every few days from all the bottles showed their contents to be sterile. Histological studies show that while there is no evidence of bacteria, there is evidence of autolytic changes, since some normal cell constituents are entirely lacking. It would seem that chloretone is one of the few substances (in weak dilution) that will allow autolysis to proceed under sterile conditions.

Conclusions. (1) Chloretone in saturated aqueous solution exerts a definite bactericdal action at all temperatures. (2) Chloretone in saturated aqueous solution prevents the development of the common molds. (3) Chloretone solution is not suitable as a fixative for histological materials. (4) Chloretone solution while acting as a bactericide, does not inhibit autolytic action as evidenced by our histological findings. (5) Chloretone solution is a desirable agent for preserving glands and gland extracts from which the active principles are to be obtained.

The outlook for chemotherapy in the chemical industry of America: C. L. Alsberg. (By title.)
Blue eyes: W. D. Bancroft.

Charles L. Parsons,
Secretary

(*To be continued*)

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